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ABSTRACT

These proceedings result from a symposium designed to provide a forum for the consideration of major social issues confronting science today. Participants (including scientists at different stages of career development from undergraduate concentrator to Nobel laureate) discussed issues related to the scientist's responsibilities as scientist and citizen, the nature and limitation of objectivity, and the inclusion of women as well as men in the world of science tomorrow. The papers presented at the symposium and included in this document are: (1) "Meritocracy and Marginality: Women in Science Today and Tomorrow" (Jonathan R. Cole); (2) "Status of Women in the Physical Sciences, Mathematics and Engineering" (Rhonda Hughes); (3) "Critical Filters in Science Careers" (Bonnie Spanier); (4) "Objectivity in Clinical Trials" (Sonja M. McKinlay); and (5) "The Cult of Objectivity in the Physical Sciences" (Jill C. Bonner). Also included is a report, "Ethical Choices for Science Panel," a summary of panel discussions which centered on Everett Mendelsohn's discussion of "Nuclear Energy, Weapons, and Scientists" and Miriam Schweber's report on "Ethics and DNA Research." These discussions focused on the dichotomy that exists between the acquisition of scientific knowledge and its subsequent application. (JN)

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Symposium Proceedings

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CHOICES FOR SCIENCE

Proceedings of Symposium

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April 11, 1980

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Introductory Comment

In planning for CHOICES FOR SCIENCE, the Bunting Institute had two principal goals in mind: (1) to provide a forum for the consideration of major social issues confronting science today and (2) to ensure that participants in the symposium would be scientists at different stages of career development from undergraduate concentrator to Nobel laureate. In selecting issues for discussion--scientists' responsibilities as scientists and citizens, the nature and limitation of objectivity, and the inclusion of women as well as men in the world of science tomorrow--we anticipated and encouraged diversity of views. Some of this diversity is conveyed in the papers presented during the symposium and collected in this volume.

I am grateful to the symposium participants and to RCA Corporation for their contributions to the success of CHOICES FOR SCIENCE.

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Meritocracy and Marginality: Women in Science Today and Tomorrow*

Jonathan R. Cole

INTRODUCTION

I want to thank you for inviting me to give this talk, and for your willingness to entertain the thoughts and speculations of one who might be considered by some as an "outsider" among "insiders" (R.K. Merton, 1973).

I must confess at the outset that since I do not know whether or not to assume that you are familiar with my book Fair Science, I feel a bit like the unfortunate animal in the 14th-century fable of "Buridan's Ass" that is caught equidistant from two bales of hay, starving to death because it is unable to decide in which direction to move. Therefore, I will proceed this afternoon by reporting some material that you will find in the book, and other materials that you will not, in an attempt to address the subject of marginality and meritocracy for women in science.

It might be useful for us not only to consider the question of where women in science stand today, and what their futures look like, but also to take a glance backwards in order to gain some perspective on where we have come from. Thus, the structure of this talk: first a brief look at some historical antecedents; then a look at where we are; and then an all too brief look at some problematics, or puzzles that remain to be addressed and to be solved. In the process, I hope that we can focus on problems of marginality and meritocracy in the scientific community, and indicate not only what we have limited knowledge of, but also what we simply know little about.

Our observations about women in science, both yesterday and today, result to a notable degree from the accumulation of two basic types of knowledge: what William James (1885) distinguished as "acquaintance with" and "knowledge about." Acquaintance with knowledge "involves direct familiarity with phenomena that is expressed in depictive representation," or what Max Weber referred to as "verstehen." Knowledge about "involves more abstract formulations which do not at all 'resemble' what has been directly experienced" (R.K. Merton, 1973, p. 133). Plainly, both forms of knowledge contribute to a full understanding of social phenomena such as the place and treatment of women in science. My knowledge, which derives to a significant degree from quantitative, empirical inquiries about the place of women in science, must surely be classified more as "knowledge about" than as "acquaintance with" knowledge. With this in mind, consider several features of the attitudes toward women and their structural position in science around the turn of this century.

* Support for this work has been obtained from the Ford Foundation grant to the Program in Sex Roles and Social Change at Columbia University's Center for the Social Sciences.

A GLANCE BACKWARD AT MARGINAL WOMEN

Until the twentieth century, science was populated almost exclusively by men, and so the phrase "men of science" was almost equivalent to the non-sex-linked tag "scientists." What is remarkable is that since the turn of the century there has been relatively little change in the proportion of scientists that are female.

If there have been few women in science, there have been even fewer among the scientific elite. The rolls of National Academy of Sciences and the list of Nobel laureates register very few women. As of 1977, less than 10 of the approximately 900 members of the National Academy were women; only five of the 281 scientists who had received the Nobel Prize were women (H. Zuckerman, 1977). How can we account for these facts? Furthermore, why have there been so few women in science (Rossi, 1965)?

While we have observed over the past 15 years marked increases in the proportion of women entering medical, law and business schools, so that they now represent roughly one-third of the population of those professional schools rather than a tenth, we have not witnessed a concomitant increase in the proportions of women in the natural sciences. To cite only a few statistics prepared by the National Research Council, in 1970 only 2.7 percent of the physics and astronomy doctorates were earned by women; by 1978 that proportion had increased to 4.9 percent -- still less than one-twentieth of the total. Similarly, in 1970, 8.1 percent of the chemistry Ph.D.'s went to women; by 1978 that figure stood at 12.7 percent. More dramatic gains were made by women in both the biological and the social sciences: an increase from roughly 15 percent to 25 percent in the biological sciences; and an increase from roughly 20 to 30 percent in the social sciences, although only 12 percent of economics Ph.D.'s went to women. In short, when we examine the figures at the point of entry into the scientific community -- the awarding of the Ph.D., we find that women comprise a very distinct minority of members. Furthermore, these proportions do not seem to be following the same trajectory as those in other professions.

Two basic questions follow. First, why have so few women chosen science as a career? Why has this underrepresentation persisted over time? Second, why have so few of the women who have entered science achieved notable distinction and rewards? Is this the result of gender discrimination? Or put more broadly, what are the cultural, social, psychological, and economic forces that influence the career paths of scientists, and female scientists in particular?

These are different yet plainly interrelated questions. They are important ones for us to attempt to answer, and although they are rapidly becoming old questions, we are only beginning to do the hard work necessary for discovering the answers to them. Most of my own work has dealt with the situation of women once they already have overcome the formidable obstacles to becoming scientists. I have concentrated on the "survivors," if you will. I shall spend most of my time talking about the questions of treatment of women in science, but I would also like to discuss briefly the question of why so few ever get there.

Simply put, until recently women were considered unfit for scientific work -- and certainly for creative scientific work. Consider only several examples of such opinion.

Alphonse de Candolle devotes only two pages to the place of women in science in his otherwise extraordinary 1885 work Histoire des Sciences et des Savants depuis Deux Siecles. Candolle remarks that the female mind

takes a pleasure in ideas that are readily seized by a kind of intuition; a mind to which the slow method of observation and calculation by which truth is surely arrived at are not pleasing. . . . Add to this a feeble independence of opinion, a reasoning faculty less intense than in man, and finally, the horror of doubt, that is, a state of mind in which all research in the sciences of observation must begin and often end. These reasons are more than sufficient to explain the position of women in scientific pursuits. (Candolle, 1885)

Candolle's ideas were not exceptional in their time. Within the scientific community in general, a significant research effort was being devoted to establishing the intellectual and psychological inferiority of the female. Extensive work was carried out on cranial volume and its relation to the lower average weight of her brain. As might be expected, evidence contrary to the "desired" outcome continually confronted these investigators. These various attempts to prove a physiological basis for the lack of female accomplishments in the arts and sciences led, of course, only to dead ends. Yet when at the turn of the century the German scholar Paul Moebius published his The Physiological Feeble-Mindedness of Women, there were fewer attacks on the conclusion than on the causal argument.

Lest I give you the false impression that my sociological brethren were immune from such sensibilities, let me assure you that our founding fathers were not. For instance, Emile Durkheim observed in Suicide:

It is said that woman's affective faculties, being very intense, are easily employed outside the domestic circle, while her devotion is indispensable to man to help him endure life. Actually, if this is her privilege it is because her sensibility is rudimentary rather than highly developed. As she lives outside of community existence more than man, she is less penetrated by it; society is less necessary to her because she is less impregnated with sociability. She has few needs in this direction and satisfies them easily. With a few devotional practices and some animals to care for, the old unmarried woman's life is full (Durkheim, 1897:215-216).

Herbert Spencer, whose work had an extraordinary impact around the turn of the century, held even more dramatic biologically deterministic views. Spencer believed that women represent "a somewhat earlier arrest of individual evolutions." And finally, Auguste Comte, the putative father of sociology, worked out an elaborate rationalization of the intellectual inferiority of women, at the same time putting them on a moral pedestal in order to keep them in their place:

Sociology will prove that the equality of the sexes, of which so much is said, is incompatible with all social existence, by showing that each sex has special and permanent functions that it must fulfill in the natural economy of the human family, and that concur in a common end by different ways, the welfare that results in being in no degree injured by the necessary subordination, since the happiness of every being depends on the wise development of its proper nature. . . . The social mission of woman in the positive system follows as a natural consequence from the qualities peculiar to her nature. . . (Auguste Comte, 1975).

I need not review here the history of the protective legislation in the United States, since *Muller v. Oregon* in 1908, to see further the stilted attitudes and beliefs about the capacity of women to handle work outside of the home in general and their work in science in particular. Allow me to skip quickly to one final example of attitudes towards women in science in the early part of the century. Consider the autobiographical reflection of Otto Hahn, the renowned physicist, about his early collaborations with his long-time colleague and prominent physicist, Lisa Meitner. To be sure, Hahn speaks of life in Germany, but there is ample evidence that conditions were much the same for women working in American science. He recalls the difficulties Meitner faced in pursuing her research goals:

The beginning was difficult for her. Emil Fischer, the director of the Chemical Institute (at Berlin), did not then accept women, but he did make a concession in her favor. With the condition that she was not to enter the laboratories where male students were working, she was permitted to work with me in the wood shop. In 1907 this was a really large concession . . . and in time he also developed an attitude of fatherly friendship toward Lisa Meitner. But the rule that she had to stay in the wood shop (which was later extended to include another basement room) remained in force (Otto Hahn, 1966).

In short, women who entered science in the early part of this century held distinctly marginal positions -- they were "marginal women" in the sense that Everett Stonequist and those that followed him applied the concept to men. As one of those followers, Robert Park, said, "the marginal man" is "one whom fate has condemned to live in two societies, and in two, not merely different, but antagonistic, cultures" (Stonequist, 1937). Women who entered science were at once rejected by those in the general culture who felt that such activities were inappropriate for women to pursue and not accepted as full members of the community into which they were entering -- the scientific community.

Women have, then, traditionally faced formidable barriers to becoming fully productive members of scientific community. They have faced what Harriet Zuckerman and I have called a "triple penalty," consisting of the following components:

- one, that science is culturally defined as an inappropriate career for women, that few women are recruited into science and few would seek it out;
- two, that those who have surmounted the first barrier, continue to be hampered by the belief that women are less competent than men at science (Whatever the validity of this belief, it contributed to women's ambivalence towards work, and reduced motivation and commitment to scientific careers); and
- three, that women had to overcome significant amounts of discrimination against members of their sex within the scientific community (H. Zuckerman and J. R. Cole, 1975).

Women who even managed to overcome the initial hurdle of cultural resistance to scientific careers for women, and who were able to make it to the starting line with Ph.D.'s in hand, were in a true sense of the term "survivors." And while I believe much has changed since the early part of this century to transform women from marginal figures in science into fuller partners with men in the scientific community, it is plain that women scientists, even today, are still "survivors." We shall return to this matter, but let me simply indicate here that the point of departure for analysis of where women stand today in science is that historically, women have been outsiders looking into the scientific community; they have been marginal figures. And once admitted through the gates into the community they were subjected to both formal and informal discriminatory behavior.

WOMEN IN CONTEMPORARY SCIENCE: PROBLEMS OF STATUS INEQUALITY¹

If this represents the historical frame, we may ask whether the same barriers to gender equality continue to exist. Consider the following queries:

¹This section draws heavily upon materials presented in Fair Science (New York: Free Press, 1979). For detailed analysis of the data that led to the conclusions presented here the reader is directed to the appropriate pages in the book.

- 1) How much evidence is there that gender-based discrimination exists in contemporary science?
- 2) Is gender discrimination, if it is found to exist, uniformly distributed throughout the scientific community?
- 3) Does gender discrimination obtain when we consider access to higher education; when we consider receipt of scholarships and fellowship support; when we consider initial job location and hiring practices after the Ph.D.; when we consider promotion to higher ranks and tenure; when we consider the receipt of research funds; when we consider the attraction of outstanding students and collaborators; when we consider access to outlets for scientific publication; when we consider access to masterful teachers; and finally, when we consider equal pay for equal work?

Questions about the level and intensity of gender-based discrimination in science must be viewed in the context of a substantial body of research over the last decade that has shown science to be a highly meritocratic social system (R. K. Merton, 1973; H. Zuckerman, 1970; J. Cole and J. Cole, 1973; J. Gaston, 1973; J. Gaston, 1978). There has been remarkably little evidence that has been brought forth to suggest that there is substantial bias in the meting out of rewards and recognition in science. Science seems to be fair. To be sure, science has its seamy side: its fraud, plagiarism and particularism (H. Zuckerman, 1977). But to remarkable degree, scientists who are the most productive and who are assessed by their peers to have made the most significant contributions to their fields and specialties through their research receive the lion's share of rewards. But there is a catch. These findings were based almost exclusively upon the study of male scientists -- of "men of science," and particularly physics and chemistry, where large-scale, empirical inquiries using random sampling techniques rarely turned up any women in the first place.

So the question I have faced is: To what extent is the position of women in science an anomaly; to what extent are women denied access to positions and to rewards in the scientific community; to what extent are they subjected to gender discrimination?

Before reporting to you some of the fundamental results of my empirical research, I must openly acknowledge the limited state of our knowledge about the measurement or estimation of discrimination.² Although each of us probably feels that we "know discrimination when we see it," "verstehen" knowledge is not the same as even rough estimation. And when we examine the literature on discrimination, whether we look at the work of economists such as Gary Becker, or sociologists such as Robin Williams or Hubert Blalock, we see that there exist not only multiple definitions of discrimination, but more importantly, few, if any, "direct"

²For a full discussion of problems of the measurement of discrimination, see Fair Science chapter 2.

measures of discrimination. Virtually all quantitative, empirical studies of discrimination, including my own, measure it not through direct observation or measurement, but residually. This is an important point to bear in mind, and indeed an important limitation in studies such as my own -- a limitation that requires us all to be organized skeptics about our results.

Discrimination can not be equated with inequality. There can be a great deal of inequality in an institution, as there surely is in science, without that inequality resulting from discriminatory behavior. Zero-order correlations, one-dimensional views of the world, can tell us next to nothing about the presence or absence of discrimination. In fact, they can be totally misleading. It may seem paradoxical that the presence of formal, statistical equality can itself result from discrimination. For if one individual or social group "outperforms" another, without that performance differential itself resulting from discrimination, then the absence of differences in rewards may actually signal discrimination.

Furthermore, we must not think that "naive residualism" has no practical outlet -- that it is not applied in the world beyond these Ivy walls. To cite only one example of its use, the basis for establishing "prima facie" cases of discrimination in litigation under Title VII of the 1964 Civil Rights Act is just such simple inequalities. Of course, most social scientists today employ what I have called elsewhere "sophisticated residualism." They start with an inequality, perhaps income differentials between men and women, blacks and whites, and so on, and then they introduce multiple variables that they believe can explain the original difference. The "left-over" difference, or the residual difference, is then taken as an estimate of "discrimination." Is this measurement procedure flawed? Yes. Are there better techniques for measuring discrimination? Apparently not, or at least none that have found even limited application.

Let me turn to results of what I have found about the position of women in contemporary science. How meritocratic is science vis a vis its female members? I wish the answers were simple, but they are not.

I studied more than 2,000 men and women scientists. They were matched initially in terms of the university from which they received their degree, their field of activity, the year that they received their degrees, and the scientific specialty in which they did their doctoral work. I traced the careers of these scientists for 12 to 20 years. I collected data as well on aspects of the scientists' social backgrounds, including their IQ's, marital and family statuses; on aspects of their career histories, such as their job changes, dates of their promotions, prestige of their affiliations, honors and awards they received, and promotion to different academic ranks; as well as on their publication histories and on the patterns of citations or references to their work by others in the scientific community. Finally, I have collected data on the "reputational standings" among a group of peers for a sample of roughly 600 men and women scientists. Standard multivariate techniques were used to analyze these data.

Let me start with the bad news that may not be news to any of you. Analysis of these data allowed me to conclude that there is evidence, today and yesterday, of significant gender-based discrimination in the promotion of female scientists to tenure and high academic rank. Even after I have taken into account many other factors, such as career interruptions and the quantity and assessed quality of research performance of the men and women, I find that women are still less likely than men to be promoted to high academic rank (J. Cole, 1979, pp. 56-58). And when they are promoted, it is not apt to happen as quickly. This finding should not be minimized. It is important because tenure and high rank not only represent security in the world of academic science, but also are requirements for full participation in the inner circles of scientific activity. Furthermore, academic rank plainly can influence income differences between men and women. In short, there is often an indirect influence of discrimination in promotion on salary differentials. Finally, high academic rank can have secondary and tertiary consequences for other forms of recognition and for the acquisition of resources necessary to carry out productive scientific work.

The evidence that I have collected suggests that the pattern of promotion to high rank, in particular the evidence of discrimination against women in this form, has persisted for the past 50 years at much the same level (J. Cole, 1979, pp. 219-225). While we shall see that there is evidence of substantial progress in other areas, there has been little movement in this one. This should have the "click of reality," as one friend of mine calls it. If we simply look around us at the elite institutions in the United States, it becomes fairly clear that there is a striking paucity of women in lofty positions at the Harvards, Columbias, Yales, and Stanfords of the academic world, even when we take into account the general underrepresentation of women in science. This absence of substantial headway in the promotion of women to tenured positions at major departments of science cannot be attributed solely to their differential rate of production of science or to any demonstrable differences in aptitudes. It is at the point of promotion to tenured ranks that affirmative action pressure should be intensified.

Now for some better news. In almost all other areas of the scientific career following the Ph.D. of men and women scientists, there is little evidence of substantial formal status inequality. There simply is little evidence of gender-based discrimination in these other features of the opportunity structure of science. Consider a series of findings both from my own work and from that of other scholars of the subject.

First, let me dispel tentatively any notions that females who enter science are not as "able" as their male counterparts. Although there is a paucity of good indicators of "scientific aptitude," at least in terms of measured IQ, women in science are certainly every bit the equal of men. In fact, when I compared the IQ scores of male and female Ph.D.'s earning degrees at science departments grouped in terms of their assessed quality, women consistently had slightly higher IQ's at every level of department quality (J. Cole, 1979: 159-160; L. R. Harmon, 1963).

Second, there is no evidence that women are systematically discriminated against in admission to graduate science departments. Women are admitted to science departments roughly in proportion to their applications.

Third, there is no evidence that women are systematically discriminated against in the receipt of graduate fellowships to pursue careers in science or in receipt of post-doctoral fellowships (J. Cole, 1979: 74; B. Reskin, 1976). Again, data from a variety of sources suggest that they receive fellowships in rough proportion to their applications, and there is no further evidence that the average quality of the male and female applicants differ to any significant degree.

Fourth, when we examine the point of entry into the scientific community, or into the academic science labor market, there is no evidence that women are short-changed in appointments to the "distinguished," "strong," and "very good" departments of science in their fields. There simply is little statistical evidence that would suggest that appointments at the level of assistant professor are meted out on the basis of gender (J. Cole, 1979:54-55). To be sure, when we look around us we will not see anywhere nearly as many women in departments of chemistry or biology or astronomy as men, and this will be true at the assistant professor level as well as at higher ranks. But that observation does not signal discrimination, since there are far fewer women Ph.D.'s than men who are entering the academic labor market in any particular group of Ph.D.'s. I have found this pattern to hold regardless of the scientific field that I have examined -- whether it be for the natural or the social sciences. Some observers might prefer to see the spectre of discrimination everywhere, but the evidence does not bear out such simple notions -- at least basing such assessments on the type of materials that I have gathered.

Fifth, despite the paucity of women who have gained the most prestigious of all scientific honors, such as the Nobel Prize, the evidence suggests that, on average, women in the 1958 cohort are no less apt to receive honorific recognition than are their male counterparts. When we examine the probabilities that men and women scientists who entered the scientific community in 1958 have received awards for distinguished work, we find no meaningful differences in the probabilities for men and women (J. Cole, 1979:58-62).

This finding highlights a point often disregarded in examining women's place in science. Most of us have been trained to look only at the elite of science. We pay little attention to the rank-and-file. There are good reasons to dwell on the elite. The overwhelming majority of "important scientific discoveries" are produced by them. But if you examine the conditions and life chances confronting most scientists you find that few can expect to earn any significant honors -- few women or men will gain any formal honorific recognition. This underscores the distinction between the analysis of aggregated and disaggregated units.

Let me digress here for a moment. Very different perceptions about levels of discrimination can be obtained by concentrating on different units of analysis; by the degree to which we focus on the "whole" or "the average," rather than on particular sectors of the scientific community. What might be true, on average, may not be true of course for a small fraction of the whole, such as the super-elite institutions, or the most prestigious prizes or awards. There may not be any difference in the probability of men and women obtaining initial first jobs in science at departments of prestige, but there may be very different probabilities for them at Columbia or Harvard. In fact, one of the drawbacks of larger statistical analysis of discrimination in science is that the focus is most often on larger statistical aggregates, rather than on comparisons of specific sub-classifications. Thus, even when I examine departmental affiliations of differing ranks, I have grouped the departments according to either the Alan Cartter categories of "distinguished," "good," etc., or the Roose-Anderson classes that contain as many as fifteen or twenty universities, rather than focussing on the one or two cases at one end or the other of the prestige continuum.

Sixth, I have found significant differences in the average "reputational standing" of men and women in the same scientific fields (J. Cole, 1979:chapter 4). In my work, reputation is measured by the appraisals by "peers" of research contributions made by a stratified random sample of scientists who work in the same field or scientific specialty. Incidentally, there is no evidence that men and women differ significantly, on average, in their assessments of women scientists, although women are somewhat more likely than men to select other women to be among the two or three most outstanding contributors to their fields over the past decade. On the basis of simple comparisons between men and women scientists, there seems to be a "cost" of being female in the process of building scientific reputations. But if we take into account the research performance of men and women scientists, in terms of both the quality and quantity of research output, the differential disappears almost completely. In short, highly prolific female scientists have similar reputational standings among their colleagues as do roughly equally prolific male scientists.

Seventh, inequalities in salaries of men and women scientists continue to exist, but recent larger-scaled studies by Centra (1974) and by Bayer and Astin (1964, 1975), among others, suggest that the earnings ratio of females to males, after taking into account a variety of explanatory factors, including research productivity, exceed .9, that is, they virtually disappear. For example, Centra's 1974 study found that male and female full professors who have been employed at universities for only 5 or 6 years have essentially identical incomes (women earn 98.5 percent as much as men); for those employed from thirteen to fourteen years, women's salaries are 95 percent of men's. Alan Bayer and Helen Astin in an earlier study (1964) found that women earned 92 percent of what men were earning. At universities, however, men and women in lower ranks earned roughly the same salaries (99 percent); within higher ranks the ratio was 91 percent. Similar results obtained when the focus shifted to colleges.

More sophisticated recent work by Bayer and Astin (1975), strongly suggests only small salary differences between men and women when we use a multivariant model to explain differences. Bayer and Astin found a $-.04$ partial correlation between gender and salaries after controlling for nine variables in a multiple regression equation. This could easily have been a spurious association had not Bayer and Astin teased out the influence of differences in academic rank, since there is good evidence of gender discrimination in promotion to high rank. But even without controlling for academic rank, they still found a partial correlation of $-.05$ between sex status and salary. In fact, female to male earnings ratios in academic science are considerably higher than in any other institutional sphere in which men and women's earnings have been compared (Lloyd and Niemi, 1979), where ratios from $.6$ to $.8$ are frequently observed even after taking into account multiple explanatory variables.

Eighth, the social processes and background characteristics by which successful scientific careers are made and by which reputations are built are almost identical for both men and women. That is to say, the very same forces that seem to determine both positional recognition and reputational recognition for men, also determine in roughly equal weight these forms of recognition for women. For instance, the quality and quantity of research publications have roughly the same effect in influencing the recognition of women as they do for men. And for both men and women, the quality and quantity of research performance are the strongest determinants of various forms of scientific recognition.

This brings me to a ninth, and in many ways the most intriguing and puzzling result of my labors -- one which continues to interest me a great deal. For every cadre of men and women scientist Ph.D.'s for whom I have collected data, there is a consistent and patterned difference in research performance. Female scientists tend not to publish as much science as males who are of equal professional age, who come from similar educational backgrounds, who are in the same fields and specialties. This result holds for every group of male and female scientists that I have studied since the turn of the century. Furthermore, these patterns of productivity differential have been observed by many other investigators, as well. (J. Cole, 1979; A. E. Bayer and H. S. Astin, 1968; B. Reskin, 1978; J. A. Centra, 1974; among others.) It is also apparently the case that the research published by women has, on average, less impact on the development of knowledge, as gauged by the number of references or citations to their work, than does research published by men.

Research productivity in science has received generous attention. It is well known, for example, that scientific research productivity is highly skewed: about 10 to 15 percent of all scientists produce roughly 50 percent of all of the scientific literature (D. Price, 1963). Most scientists produce only three or four papers in a scientific career. In any given year, the vast majority of scientists, even those holding positions at universities, will not publish a single paper. This holds for men and women alike. The skewed distribution of research productivity also is similar for both men and women. If we examine the research patterns of the male scientists who

received their Ph.D.'s in 1958, we find over a 12 year span a Gini concentration ratio of .502 for the men. When we look at the women in the same sample we find almost the identical ratio, .504. In short, among women scientists, about 15 percent produce 50 percent of the total output, or about the same proportion as obtains for men (J. Cole, 1979; 63).

Most importantly, there is a moderate and consistent correlation between gender and research performance, in the neighborhood of .3, with men consistently publishing more than women. When we focus on the impact of published research, similar differences are found. Finally, when we examine these performance differentials over time, we find that differences in research productivity between men and women Ph.D.'s seem to increase over the course of the scientific career, and that the correlation between early and later career productivity is significantly higher for men than it is for women.

How can we account for these patterned differences in research performance between men and women? There have been several hypotheses, and many speculations, but few satisfying answers. Consider just several proposed explanations for the differences.

Noting these patterned differences in published productivity, it often has been asserted that the multiple role obligations of female scientists, particularly those of wife and mother, detract from the time needed to be more productive scientists. Surely the effects of marriage and families differ for men and women, even in today's world, and indeed this is a plausible hypothesis. Yet the data don't seem to support it.

My data show that not only does marriage and family life fail to impede scientific productivity of women, but it seems to be related to small increases in research performance. Women scientists who were married turned out to be significantly more prolific than those that were not; and women who were married with either one or two children were slightly more scientifically productive than women who were unmarried, and only slightly less scientifically productive than those who were married without children. However, there is a limit to all of this. Once a woman has had three or more children there is decline in research output, but not to a point that it is significantly lower than that which is found among the unmarried women. Such results fly in the face of conventional wisdom and we should therefore approach them with caution. Of course, *a fortiori*, we can create plausible explanations for this outcome. Emile Durkheim might rejoice at such findings, since he would have liked us to believe that marriage and the family actually have a stabilizing, integrating, and cohesive effect on individuals. The data appear to suggest that despite the hassles and claims of marital and family obligations, they are not of a form that adversely influences research performance, at least no more so than the strains associated with being single.

The relationship between gender, marital and family life, and scientific research performance typifies many other recent findings: they are somewhat counterintuitive, they are puzzling, and since they are based upon limited data they call for important further inquiries. Consider several questions that might be put to me about these findings. Does this pattern hold for women in all age groups? When we compare the women who are married with families with those who do not have families, are we comparing the "same" women? Perhaps the women who continue to be active scientists and who are simultaneously able to manage their family lives, are quintessential "superwomen." Or perhaps they have more help in carrying out their multiple tasks. Furthermore, if we look at the career histories of these female scientists, what are the short-run vs. longer-run effects of having children on patterns of research publications? Are there temporary gaps or declines in scientific research productivity among these women while they attend to their other forms of productivity?

In brief, we simply do not yet have answers to these questions. Some observers of science believe that the gap in research productivity between men and women is itself the consequence of subtle and not so subtle discriminatory practices within science; the result of limited and impeded access to the resources that are needed to carry out scientific research, limited access to publication outlets, limited access to government and other forms of support for scientific research, limited access to the best "human capital" available within the graduate student population, and limited availability of willing colleagues with whom to collaborate; and the result of excessive non-research demands made on women faculty members in the forms of higher teaching loads, more extensive committee assignments within departments, etc. There are almost no data available to test these conjectures.

Other observers have hypothesized that the productivity differential results from the different structural positions that men and women of science occupy. Women are slightly more apt than men to be located at teaching rather than research oriented institutions. But preliminary examination of data that compare men and women at similar settings does not show more than a minor reduction in the productivity differentials.

Still other observers of science might have us believe that the differential is a function of motivational problems that result from different reference groups. These observers argue that women who achieve Ph.D.'s in science represent more of an elite group relative to other women than do their male Ph.D. counterparts. For female scientists, who have had to overcome cultural and institutional constraints, the Ph.D. becomes and end-in-itself; for men it simply represents a union card -- a temporary way station toward higher positions in the scientific community. There is limited information to test these conjectures. In short, we don't really understand the social processes at work that influence the emergence and maintenance of these patterned differences in research productivity.

PROBLEMS OF STATUS INEQUALITY AND CITIZENSHIP

These, then, are some highlights of my empirical studies. On the basis of these data, we might conclude that status inequalities between men and women scientists have been reduced sharply over the past 50 to 75 years. Women have moved from marginal positions, which hovered at the outskirts of science, toward the inner core of the community. In fact, there have been substantial reductions in most status inequalities, of course, with the critical exception of the area of promotion to high rank. And, I believe, the quality of experiences has improved substantially. It is in the domain of informal activities in science that the biggest gaps between men and women remain. It is in the more intangible set of experiences associated with doing science from day-to-day that women today rightly feel most excluded.

To say that women of science have now entered the central scientific community, and that they have achieved formal equality with men in terms of many measurable aspects of the reward system, does not say that the opportunity structure for men and women interested in science as teenagers is equally open. Nor does it say that the women who have chosen science have an equal chance of winding up in the "inner circles" of science, or that they will be equal participants in the "invisible colleges" of the scientific establishment. No. Resistance to full participation, to full citizenship of women in the scientific community, continues to exist. Some of the types and sources of that resistance are known, but I believe that many of the sources are little understood, if even correctly identified.

Young women interested in careers in science today continue to have more formidable high hurdles to negotiate than do young men. And women already in science continue to be faced with obstacles to full citizenship, but not as much in the formal sense of exclusion as in the informal sense of limits on the behavior that will allow them to achieve up to their capacity.

In Fair Science, I suggested that some of the incredulity about some of these findings resulted from "the haunting presence of functionally irrelevant statuses."³ I hypothesized that the salience of gender in contemporary society leads both men and women to construct post factum causal explanations of career decisions and formal inequalities in terms of gender. Gender is perceived as the principal reason for differences in status attainment, and other, alternative explanations are discounted. I now believe that the reasons for the incredulity go beyond this, and have to do, at least in part, with the type of knowledge that can be obtained through the use of current quantitative, social science methods.

³ A full discussion of the concept appears in Fair Science.

I believe that typical quantitative methods can estimate rather well features of the formal aspects of "citizenship" in science. They allow us to estimate status inequalities in affiliations, in awards and honors received, in academic rank, etc. But these techniques, at this stage of their development, do not allow us to measure adequately other, informal aspects of "citizenship."

Until the early part of this century, the inequalities in positions held by men and women scientists were so dramatic that they resulted in status-set configurations that were almost totally dissimilar. These differences in status extended even beyond science into aspects of marriage and family life. Women scientists, for example, rarely were married, since marriage itself could have spelled the end of their careers. Men controlled all of the "means of scientific production"; they dominated almost all positions of authority and power; they completely controlled the structure of formal opportunities. A totally asymmetric relationship existed. At that point women were totally marginal figures in science.

I believe that many of those fundamental cleavages began to break down early on in the century, but sharp differences in social status remained. In most ways that status inequality could be measured, substantial gaps in position, rank, and salary persisted. Although women had moved into the community of science, they were not of that community. If there has been movement toward greater application of meritocratic principles over the past 25 to 30 years, it has taken the form of reducing the level of formal status inequality between men and women scientists.

What I do not believe has been achieved to any significant degree is the achievement of full citizenship for women in science.

When I speak of citizenship, I have in mind T. H. Marshall's concept of social citizenship. The social element in citizenship involved "the right to share to the full in the social heritage and to live the life of a civilized being according to the standards prevailing in the society." (T. H. Marshall, 1965, p. 78). Many women continue to be excluded from the very activities of science that allow for full participation and growth, for productivity and change. These are, by-and-large, the informal activities of science; the participation in the heated discussion and debates in the laboratory; the inclusion in the inner core or the "real" invisible colleges of science; the full participation in the social networks where scientists air ideas and generate new ones. These relationships involve what Mark Granovetter (1973) has called "the strength of weak ties." These are also the close collaborative relationships that grow up over time, that help to shape scientific taste and sharpen the eye for a good research problem. Women perceive in significant numbers that they remain excluded from those activities that really define full membership in the community. I believe that there is much merit to those perceptions, although we have almost nothing more specific than anecdotal, autobiographical evidence to support them.

In fact, I believe that the differences in status and "social citizenship" explain some of the apparent clash between the knowledge generated by "verstehen" and knowledge generated by means of larger surveys. Why do the findings reported in my book often fail to elicit in many women scientists that "click of reality?" Part of this results from the limits to the two forms of knowledge. The quantitative methods that are used to measure features of scientific careers can begin to estimate the extent of status inequalities, and they can begin to account for those that do obtain. But these are differences that obtain in formal recognition. Verstehen knowledge captures aspects of citizenship that quantitative data in their current form are simply not able to measure. The value of knowledge acquired through direct experience is that it allows us to focus on particular, detailed forms of inclusion and exclusion within the scientific community. Its shortcoming is that it cannot accurately represent as can larger studies using multivariant analytic techniques the general level of status inequality that obtains in the larger social system.

In my final set of observations I would like to identify several little understood processes that must be addressed if full citizenship and indeed real meritocracy are to be achieved in science.

LOOKING FORWARD: PROBLEMS FACING WOMEN IN SCIENCE

1. Processes of self-selection and social selection.

Most of our intellectual energy in examining the position of women in science has been spent on the social processes within science that act as impediments to the career development of women. For the most part, we have looked at processes of social selection, by which institutions and organizations make selections for positions, awards, promotions, salaries, among competing candidates. And more specifically, we have attended to the question of whether these processes of social selection within the scientific community operate fairly, meritocratically, with or without discriminatory intent. We have neglected a host of extremely difficult questions about barriers that face young women who are interested in science, but who turn away from it as youngsters or as college students. We have concentrated, as I said earlier, on "survivors." In a sense, we start looking for problems at the finish line -- after the race is already run. We have neglected to study the forces exogenous to science that make the low proportion of women in science virtually inevitable. We need not rejoice over science approaching its universalistic ideal when only four or five percent of the total population of physicists are women in the first place.

Perhaps the single most important question that we can ask is: What forces persist in the general culture, in the general value system of American society, in the American family, in our schools, in the presentation of professional careers by the mass media, that turn young women away from the starting line?

Young women tend to self-select themselves out of science at early ages. And we know remarkably little about the factors that lead to this decision. We know that science has not been viewed as an appropriate career for women, and that women were thought not to have the capacity to do outstanding scientific work. But we do not know much about how these values are transmitted to young women. We do not know much about the intersection between culture and social structure in processing these general beliefs and values. We have no idea about the relative effects that parents, peers, teachers, guidance counsellors, and curricula have in turning young women away from science. Finally, we know little about the perceptions of science among boys and girls in early adolescence. As an activity, as an enterprise, does science have the same fascination for young girls it does for young boys? Does it trigger their imaginations in the same way? If this is not the case, why not? And if it is, how does this enthusiasm become dampened over time?

Let me be clear. Understanding the decision-making of teenagers may be even less tractable than understanding other decision-making processes. Occupational choice has had a long history of being a particularly thorny area of research. Even the ablest scholars have found it difficult to fathom why youngsters make the career choices that they do. (D. Eiduson, 1973; R. K. Merton, et al. 1957). Nonetheless, the problems are so significant, and the consequences so great, that a renewed effort must be mounted to better understand how our culture leads science to be viewed by our young women as an inhospitable and unattractive environment. Until we understand more fully the social conditions that lead young women to select themselves out of science, we will not have an opportunity to change these conditions. In the final analysis, opening up science to women may depend more on our getting a handle on these problems than on anything else.

2. Processes of Accumulation of Advantage and Disadvantage.

There is a tendency to view the position of women in science statically, rather than in terms of on-going social processes. In fact, there is substantial evidence that advantages and disadvantages in science cumulate over time. A social system that operates meritocratically at one point in time *vis à vis* a group of scientists may still discriminate against that group. Suppose the social system of science is less apt to support the education of women than men in terms of apprenticeships. If women scientists receive less support than men, it should not surprise us to find them less scientifically productive. Their futures in fact become predictable. When they come up for promotion, their publication records are carefully reviewed and found inferior to those of men with the "same" types of background, and they lose out in the academic marketplace. The self-fulfilling prophecy, which is based on the assumption that women are less motivated, less productive, and less reliable scientific risks than men, now is strikingly supported by data. Gate-keepers for resources may now present data to justify not giving equal financial support to women scientists, since they are less likely to produce significant research with the funds. And so the conditions for the self-fulfilling prophecy are reinforced. Plainly if a self-fulfilling prophecy operates at an early point in a career, a universalistic judgment later on will ultimately produce inferior status for the judged group.

Correlatively, if certain youngsters are labeled early on as potential stars, and are consequently given resources and disproportionate opportunities to study at the sides of masters, are given disproportionate opportunities to collaborate on research projects and on publications with senior scientists, it should not surprise us if later on in their careers they have more formidable research track records. They then have somewhat higher probabilities of receiving support for future research, and are apt to have received a greater proportion of honors and rewards. Of course, there is nothing wrong with such processes of accumulating advantage if initial selection is based solely on meritocratic and impartial criteria. In that event, the social system of science would be distributing resources to those who are in fact most talented and promising. However, if the initial assessment of who is apt to be a "star" is based on functionally irrelevant criteria, such as gender, then the process of accumulating advantage can begin to enhance the career possibilities of men, while diminishing the chances of women. There is relatively little information about the processes of accumulating disadvantage and how it influences the careers of women in science.

Moreover, we do not know how aspects of women's lives outside of science produce disadvantages that are felt during the course of a scientific career. Consider only one concrete example. It is apparently the case that the patterns of geographic mobility of men and women scientists differ (Marwell, et al., 1979; Committee on Status of Women, AAJ, 1979). This is especially true for married women scientists. For whatever reasons, women feel more constrained than men in shifting job locations if they are married or are part of a dual career couple. The "Committee on the Status of Women" of the American Astronomical Society addressed this matter recently:

In cases where a move was considered very difficult, prohibitively difficult, or of uncertain difficulty, 84% of the women listed employment of the spouse as the reason, while only 43% of the men gave this reason. . . . The women respondents feel much more constrained to obtain jobs in the location where their spouses work than do the men (AAS, Committee on the Status of Women, 1979, pp. 8-9 of preprint).

It is also well known that geographic mobility tends to be associated with the building of reputations in science, with the development of important social ties, as well as with tangible rewards such as increases in salaries at one's own university or college. A time honored way for scientists to obtain improved positions at "home" is to establish their market value "abroad." If women are less willing than men to entertain moves, and perhaps equally importantly, if they are also defined by others as less likely to be moveable because of a spouse, then a process has been set in motion that may reinforce the accumulation of disadvantage.

In sum, both the processes of social selection, including gender discrimination, and self selection can operate to compound initial small advantages or disadvantages into marked differences in status and rewards. We must begin to study the careers of scientists as a set of dynamic and interacting processes that can quickly transform equality into inequality.

3. Productivity Differences.

We have not begun to understand fully the productivity patterns of men and women. Are the differentials that have appeared in multiple studies of men and women scientists a thing of the past? What accounts for sex differences among academic scientists and scholars in published productivity? Do such differences in fact increase with age? Are younger women more like their age-mates among men in research patterns and performance than are older women? And what accounts for differences in the extent to which the works of men and women are cited in the research literature? Have the women's movement and "affirmative action" resulted in smaller sex differences in productivity among those educated after these historical changes? How, if at all, do the work habits of women differ? What factors determine whether a woman will be a prolific or a silent scientist? How do these factors differ among the several scientific disciplines? These are important questions for both the study of gender-based discrimination in science and the formulation of science policy.

Why is knowledge about the determinants of productivity patterns among women important for science policy? Although women still represent a distinct minority among physical and natural scientists, their proportion is growing steadily. The emergence of women as a truly significant rather than as a token proportion of the community makes questions of scientific productivity differentials all the more important. If women represent an increasing portion of the entire community, and there are forces either inside or outside of science that produce a lower than expected level of research output, what will be the consequences for the growth of knowledge in these fields? It is imperative that we identify the barriers to full productivity of women so that we can eliminate them.

4. Informal Social Networks: The Strength of Weak Ties

My work has not been finely tuned enough to identify and describe precisely the detailed patterns of social interaction and sponsorship that are an essential part of successful careers. Being part of the "proper social network," being linked to the "right" people, plainly goes some distance in determining the paths taken in scientific careers and contributes to making science fun. None of this, of course, is a substitute for talent. But given talent, it plays a significant role in launching and sustaining careers.

Consider only several questions that need to be researched. Do women have the same opportunities as men to establish "apprentice" relationships with older, eminent scientists? Whether we examine August Comte's relationship with Saint-Simon; Fermi's with Corbino, Segré's with Fermi; Otto Hahn's with Rutherford, Lisa Meitner's with Otto Hahn; Mary Whitton Calkins' with William James; or the thousands of other master-apprentice relationships, we are dealing with an important mechanism of transmitting a scientific tradition from one generation to another. Even a cursory skim of the autobiographical histories of scientists reveals poignantly how sponsorships involve jealousies, ambivalence, conflict, admiration and love; and how they are always essential in producing in young scientists a sense for a good question or a key problem, a style of doing research or theorizing, a critical stance, and a way of teaching their own future intellectual progeny.

As part of the new study of processes that sustain or set in motion an accumulation of advantage or disadvantage we must describe in detail the informal structure of activities of young men and women scientists. Are women, for example, less apt than men to spend extended amounts of time in informal scientific discourse with their teachers -- and if so, why? Are they less likely to assimilate the style of scientific work which is so important to later productivity? Do science professors attempt to place women protégées in laboratories and departments where there is a significant chance for them to work on ongoing projects or programs, to develop their own ideas, and to have the facilities necessary to translate ideas into publishable papers? Perhaps even more importantly for research performance, do senior professors engage in scientific collaboration with junior female colleagues; do they ask them to join their laboratories; do they get them invited to conferences at which they describe their work to groups of scientists who have already gained some prominence and are influential in determining the next generation of esteemed and prominent scientists; do they aid in obtaining for their female students and junior colleagues invitations to publish papers in journals, in conference reports, and in proceedings from symposia? Finally, are women more apt than men to encounter difficulties in recruiting the most able graduate students to work for them, and eventually in collaborating them?

Of course, the simple answer is that some do and some do not; but a more detailed inspection of the problem should help to establish whether for women the probabilities are lower than for men of having these informal experiences that eventually are reflected in productivity rates and higher reputational standing. More specifically, do differential probabilities exist among the most talented young male and female scientists, for whom these opportunities might actually be significant (J. Cole, 1979, pp. 130-132)?

There are no existing quantitative studies that can immediately answer these questions for us, no multiple regressions that will describe or establish the impact of these social linkages. There is work to be done here if we are to discover how the actual experiences of men and women scientists are similar and are different, and indeed whether gender is of little consequence in the development of scientific careers. It remains unclear whether or not women are less apt than men to benefit from the operation of informal networks in science. I suspect that women are relatively deprived. It is plainly an area that needs thorough investigation.

5. Measuring Discrimination.

Our attempts to measure discrimination precisely are relatively primitive. Whether we deal with impressionistic evidence provided from autobiography or with the results of larger quantitative empirical inquiries, we have at best very limited and often indirect measures of the phenomenon that we want to understand. Substantially more work must be done to discover new and improved methods of measuring discrimination.

6. Gender Discrimination in Promotion.

In the end I want to return to the beginning: the evidence that there exists significant gender discrimination in promotion. Affirmative action efforts should concentrate on this critical pressure point. Diffuse affirmative action efforts tend unnecessarily to deplete the energy that is needed to attack the major problem of discrimination in academic science. This should be the case most particularly at elite institutions of science, which have always set standards followed by others.

CONCLUDING REMARKS

Let me conclude. I believe that we have made a beginning toward true meritocracy for women in science. Historical materials and contemporary studies suggest strongly that we have moved a great distance toward equality of status for men and women. Important pockets of status inequality remain. In particular, colleges and universities have been remarkably slow to reward their female scientists with promotion to tenure and high rank. We must apply pressure to change that situation. But there are two important areas where I believe change has been even slower. Within science, the failure of women to be accepted as full citizens with equal participatory rights in the informal activities of science results in their remaining in many ways second class citizens. Such forms of exclusion and discrimination are difficult to overcome; they are not easily attacked through litigation and other formal sanctions. But they must be overcome before we can say that science truly approximates a meritocratic community, where all of its citizens regardless of gender enjoy equal rights and opportunities.

And beyond these matters of attaining equality for those already in science, the larger question for tomorrow is how do we alter American culture in such a way that science will become an attractive career for bright, young, energetic women? How can we adapt the culture and its various institutions so that they will encourage women to enter science and will not derail them through sex stereotyping and self-fulfilling prophecies? If we are looking toward the horizon, if not over the rainbow, the most pressing problem is for us to be able sharply to increase the numbers of young women who enter the community in the first place. And as I have said, we are abysmally ignorant of the social and psychological processes that influence these decisions. To remain so condemns us to being concerned about the welfare of 10 or 15 percent rather than what could and should be 50 percent of the scientific community.

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Status of Women in the Physical Sciences,
Mathematics and Engineering

Rhonda Hughes

Although affirmative action and equal opportunity employment practices as mandated by Title IX have been in effect since 1972, the number of women holding academic positions in scientific fields remains discouragingly low. Because mathematics is a critical prerequisite for any career in the sciences, and because I am a mathematician, I have chosen to seek possible causes for the current situation by examining those factors which I feel contribute to the high attrition rate for women from mathematics at every level of their intellectual development.

Already in early childhood girls are subject to the stifling influence of toys which promote passivity and conformity to traditional stereotypes. A walk down the aisle of any toy store is a grim lesson in the different expectations our society holds for girls and boys, and hence for women and men. As soon as we pass the toys of infancy, we encounter the dolls, dishes, pots, pans and irons which must dampen the scientific curiosity of their unsuspecting recipients. By contrast, the toys traditionally reserved for boys' use seem fascinating even to an adult. Although there is no conclusive evidence for differences in mathematical ability between boys and girls before adolescence¹, I am convinced that many of the attributes which are so valuable to the scientist in later life are already being programmed out of girls in infancy and early childhood.

In elementary school the conforming, nonaggressive behavior for which girls are frequently rewarded is incompatible with the demands of their innate intellectual curiosity. They may submerge their natural creativity in a trade-off for the approval of their teachers; moreover, these teachers often expect boys to do better in mathematics² and may themselves feel they have an inadequate grasp of the material. It is not altogether surprising that this climate produces girls whose participation in academically challenging courses in mathematics and science decreases sharply in high school when these courses become optional.

Other factors which may contribute to the disparity between the interest and achievement of girls and boys which appears at this level are societal expectations and peer pressure. The sexual stereotype of the woman scientist as a rather formidable individual is as persistent as it is inaccurate; because

¹ Lilli Hornig, Climbing the Academic Ladder: Doctoral Women Scientists in Academe (Washington, D.C.: National Academy of Sciences 1979), pp. 11-12.

² John Ernest, "Mathematics and Sex," American Mathematical Monthly 83 (1976) p. 599.

of this stereotype, young women fear they must forego their "femininity" in order to become scientists. In his eulogy of the eminent woman mathematician Emmy Noether, Hermann Weyl said, "No one could contend that the Graces had stood by her cradle;... if we... often chaffingly referred to her as 'der Noether,' it was also done with a respectful recognition of her power as a creative thinker who seemed to have broken through the barrier of sex...."³ I have no doubt of Weyl's respect for Noether and her work, but it is hard to imagine that were she a man, he would have said, "the Graces had not stood by his cradle...." Today, we regard his remarks about Noether's "femininity" or lack thereof as irrelevant; by increasing the awareness in girls of the diversity of people who actually are mathematicians, such stereotypes can be dispelled.

Moreover, the current popularization of math anxiety may lead girls to believe they suffer from an inevitable female disorder and may ironically reinforce existing stereotypes of women as being inept at math and sciences. Another factor is the misleading impression young women have about the degree of mathematical facility needed for their particular career aspirations. Even the traditionally female fields in the social sciences, humanities and education may now require more mathematics than was previously assumed. Lucy Sells has referred to mathematics as a "critical" filter in the job market," because women are effectively barred from a wide variety of careers due to inadequate preparation in mathematics.

As young women are beginning to avoid mathematics and science, the participation of their male peers is increasing. The aggressive behavior which was often a problem for boys in the primary grades in high school becomes an acceptable approach to problem solving. Young men who now demand challenging work are viewed by their teachers as "finally settling down."⁴ Moreover, since society views the acquisition of mathematical and scientific knowledge as an appropriate activity for men, there are no stereotypes to be fought at the expense of valuable intellectual and emotional energy.

Patricia Casserly, a research scientist at Educational Testing Service, recently found three characteristics common to math teachers in schools with strong programs for both women and men:

- (1) the teachers use older girls to counsel and tutor younger girls;
- (2) the teachers have access to their students' families; and
- (3) the teachers seem to thrive on teaching students who may be brighter than they are themselves.

This last characteristic is crucial to the young women they teach for, as a rule, gifted young women are not the delight to their teachers that young men are.

³Lynn M. Osen, Women in Mathematics (Cambridge, Mass.: The MIT Press, 1974), p. 152.

⁴"Factors Related to Young Women's Math Achievement," Caucus for Women in Statistics Newsletter, 9:1 (February 1979).

Also, girls need more realistic counselling and challenging experiences in the schools. "Math can become a natural pursuit long before girls wonder whether it's a proper subject or before sexual self-consciousness and sexual stereotyping become problems."⁵

The young woman whose enthusiasm has survived until college faces a new set of obstacles. Encountering for the first time the predominantly male scientific establishment, she may find that the shortage of women faculty members creates a less than supportive atmosphere. For example, women students often feel more comfortable seeking help from their women teachers, whom they claim are more sympathetic to their needs; the additional responsibility this implies for the woman faculty member seeking tenure is a significant issue in view of the current demands of academe. Also, research has shown that both women and men faculty are more supportive of students of the same sex⁶; hence, the scarcity of role models begins to take its toll. The relatively large proportion of women faculty at women's colleges has been suggested as the possible reason for their impressive record for producing women who are subsequently cited for career achievement.⁷

Another aspect of the women's colleges which may contribute to their success is the absence of male students in the classroom. A young woman majoring in mathematics may be the sole representative of her sex in the classroom. If she is not "one of the boys," she may miss out on the intellectual exchange among students which is so important at all levels of scientific development. The situation is exacerbated by the greater confidence men display in their abilities; as noted in the compilation of research on sex differences by Maccoby and Jacklin, men overestimate their position in the dominance hierarchy, and their sense of self-worth is enhanced by positive feedback⁸. Such self-assuredness, even in the absence of any particular intellectual superiority, is, in my opinion, a manifestation of the sense of potency one derives from a supportive environment. Whereas male mathematicians may favor the more demonstrative "male style" in which they themselves were primed since childhood, a woman who performs in the more methodical, disciplined fashion reminiscent of the behavior for which she was rewarded in the elementary school setting may find herself criticized for being "a good student, but not creative."

The woman who chooses to do graduate work in mathematics may be affected to an even greater extent by the paucity of role models. Women who have power in their departments and need not fear for their jobs not only provide encouragement and support to women students, but may enlighten male colleagues and graduate students as well. Given the current employment situation, an unfortunate woman graduate student may never encounter a prominent woman in her field. For example,

⁵ Ibid.

⁶ Hornig, p. 15.

⁷ Ibid., p. 14.

⁸ Ibid., p. 13.

in 1978-79, the percentage of Ph.D. women on the faculties of the 27 most prestigious mathematics departments was 4.5, with 2.7 tenured⁹. In my own case, the presence of tenured women was a significant aspect of my graduate school experience; the fact that I was not at one of the 27 most prestigious schools was more than offset by the more relaxed and egalitarian atmosphere which produced many women Ph.D.'s. It should be pointed out that even when suitable candidates are available, women are frequently overlooked for positions which may increase their visibility. For example, in a recent survey conducted by the Association for Women in Mathematics, of 467 journal editors, only 8 were women. When questioned about the situation, one editor replied that only "leaders in the field" were appointed to the editorial board. After a tactful reminder that one of the editors was by no means in this category, a woman was appointed to the editorial board.¹⁰

Although the percentage of Ph.D.'s in mathematics has been growing for several years, to 13.3 in 1979, women do not seem to be securing academic positions at the same rate as doctorates. For example, the percentage of women on the tenured faculty of four-year colleges and universities remained fixed at 4.4 over the two-year period 1977-79.¹¹

Once appointed to tenure-track positions, women faculty members in general climb the academic ladder more slowly than men, and they are less likely than men to achieve tenured status¹². Among the possible causes contributing to this situation is that women in general have higher teaching loads: NCES data show that 35 percent of female university faculty, compared to 53 percent of male university faculty, teach eight hours or less per week¹³. There is also concern that women's research may be evaluated unfairly; there is evidence that even when the publication rates of women and men are identical, men are promoted more rapidly.¹⁴ Examples of discrimination and underemployment of qualified women are well documented, but in the mathematical community the case of Julia Robinson, the first woman in mathematics to be elected to the National Academy of Sciences, is the most striking. Robinson was for many years a lecturer in the mathematics department at Berkeley. Unemployed at the time of her election to the National Academy, Berkeley promptly offered her a full professorship. Efforts to eliminate alleged biases have been for the most part unsuccessful; blind refereeing has been a hotly contested issue in the American Mathematical Society for several years, and was recently abandoned due to considerable opposition.

⁹ Judy Green, "Fifth Annual Report of Employment of Women in Mathematics Departments," Association for Women in Mathematics Newsletter 9:1 (January-February 1979).

¹⁰ Bhama Srinivasan, "First Report on a Project Involving Journal Editorships," Association for Women in Mathematics Newsletter 9:5 (September-October 1979).

¹¹ Green.

¹² Hornig, p. 82.

¹³ Ruth B. Ekstrom, "Women Faculty: Development, Promotion, and Pay," Findings, ETS 5:2 (1979), p. 2.

¹⁴ Ibid., p. 3

As mentioned earlier, the woman faculty member may face additional demands on her time due to the small number of women in her department. In sciences where graduate students and research grants are indispensable, the aspiring young woman often feels she lacks the professional connections which are important in the establishment of a reputation, and hence in the accrual of grants and students. One difficulty in establishing connections is that women often report feeling isolated from their male colleagues, socially and professionally, and that their colleagues may feel uncomfortable or restrained in their presence. Indeed, Edith Luchins has documented these attitudes among women mathematicians.¹⁵

Although I have said little about other areas of science, similar situations prevail. In a recent article, Vera Kistiakowsky demonstrates that the status of women in physics has changed little since 1971 or, for that matter, since the beginning of this century. With the exception of assistant professors, the increase in the percentage of women professors in doctorate-granting physics departments since 1971 is insignificant. For example, in 1971-72, 1.5 percent of all professors in such departments were women, whereas in 1978-79, 1.7 percent were women¹⁶. It remains to be seen whether the increase from 2.0 to 4.5 percent for assistant professors, as well as the increasing percentage of Ph.D.'s awarded to women, will affect the status of women physicists.

For women in chemistry, there are few models in prominent positions. As of 1977, less than 3 percent of the faculty in the 35 most prestigious chemistry departments were women; in fact, of 188 doctoral-granting departments, 114 had no women in tenure-track positions. More encouraging statistics are that in 1977 12 percent of new Ph.D.'s were women, and that some graduate schools boast female enrollments as high as 35 percent¹⁷.

The prognosis for the status of women in engineering is, perhaps, the most promising, for the number of women freshmen in engineering has risen 623 percent from 1969 to 1976. Although the effects of this increase are not yet fully reflected in bachelor's degrees, the number of women receiving degrees in engineering has increased 509 percent in this same period¹⁸.

¹⁵Edith Luchins, "Women in Mathematics: Problems of Orientation and Reorientation," Final Report on NSF Grant, Jan. 1976 (preprint).

¹⁶Vera Kistiakowsky, "Women in Physics: Unnecessary, Injurious and Out of Place?" Physics Today, Feb. 1980, pp. 32-40.

¹⁷Rebecca L. Rawls and Jeffrey L. Fox, "Women in Academic Chemistry Find Rise to Full Status Difficult," Chemistry and Engineering News, Sept. 11.

¹⁸Betty M. Vetter, "Data on Women in Scientific Research," Association for Women in Mathematics Newsletter 9:3 (May-June 1979).

Lest the future seem to gloomy, let me close by saying that there are several positive steps being taken to analyze and remedy the situation. On the West Coast, there are currently several programs underway to encourage women to study math and science. Led by Lenore Blum and others, there is a "Women in Science" program at Mills College designed to prepare students with inadequate backgrounds for calculus. The "Math for Girls" program at Lawrence Hall of Science was designed to provide an opportunity for 6-14 year olds to explore mathematical concepts in a non-threatening environment. The Math/Science Network, which grew out of these projects, has conducted several conferences designed to increase young women's interest in and awareness of careers in math and science. Recently funded by the Carnegie Corporation, the Network will disseminate its resources to provide models for such activities throughout the country¹⁹. Also, the Association for Women in Mathematics has made great strides in increasing the visibility of and communication between women mathematicians. Each year, as more women receive Ph.D.'s in the sciences and slowly climb the academic ladder, the number of role models for aspiring young women will increase, for the ultimate benefit of society.

¹⁹"Activities of Bay area Math/Science Network," Association for Women in Mathematics Newsletter 9:2 (March-April 1979).

Critical Filters in Science Careers

Bonnie B. Spanier

I will start by reading a poem by Adrienne Rich called "Planetarium". She is thinking of the English astronomer Caroline Herschel, who was born in 1750 and lived to be 98 years old, and of other women astronomers, such as Sophia Brahe, a Dane who lived in the 16th century. Sophia Brahe's brother, Tycho, is also mentioned in the poem; he was the very famous astronomer who said he counted the stars so that posterity would have fewer to count.

Planetarium

A woman in the shape of a monster
a monster in the shape of a woman
the skies are full of them

a woman 'in the snow
among the Clocks and instruments
or measuring the ground with poles'

in her 98 years to discover
8 comets

she whom the moon ruled
like us
levitating into the night sky
riding the polished lenses

Galaxies of women, there
doing penance for impetuousness
ribs chilled
in those spaces of the mind

An eye,
'virile, precise and absolutely certain'
from the mad webs of Uranisborg
encountering the NOVA

every impulse of light exploding
from the core
as life flies out of us

Tycho whispering at last
'Let me not seem to have lived in vain'

What we see, we see
and seeing is changing

the light that shrivels a mountain
and leaves a man alive

Heartbeat of the pulsar
heart sweating through my body

The radio impulse
pouring in from Taurus

I am bombarded yet I stand

I have been standing all my life in the
direct path of a battery of signals
the most accurately transmitted most
untranslateable language in the universe
I am a galactic cloud so deep so involuted
that a light wave could take 15
years to travel through me And has
taken I am an instrument in the shape
of a woman trying to translate pulsation
into images for the relief of the body
and the reconstruction of the mind.

1968

This poem embodies for me the hopes and aspirations of humans, of women, to have free choices: the desire to be themselves, to be scientists, if they want. The first line, "a woman in the shape of a monster," evokes the psychological terrors of deviance within society.

The end of the decade of the '70's saw the percent of Ph.D.'s granted to women finally come up to the level that it was in the 1920's. This shocking fact, the halt and reversal, after the 1920's, of the progressive increase seen 100 years ago, stands as a grim reminder of the ease with which society falls back (or is pushed back) to the old ways, which limit the freedom of particular groups of people.

Why do women represent only 10% of all Ph.D.'s in science and engineering, with great variation of participation in the different fields, ranging from 17% of Ph.D.'s in biology down to 7% in math and in chemistry, 3% in physics, and 1% in engineering?

Dr. Hughes has described some of the key factors in the early stages of education and training that limit access to the physical sciences; mathematics plays a crucial role. The situation in biology and psychology, in which women participate to a much greater extent, is less severe. One of many factors contributing to this is that much less math is required in these fields.

What are the other factors that act as critical filters for women entering science and developing as scientists? They are numerous, complex and intertwined. I will only touch on some of them.

The major factor is outright discrimination in hiring and advancing women through the scientific system. Let us focus on academe. Women are concentrated in the lower ranks, are not advanced as rapidly, are not given tenure, are paid lower salaries, and are concentrated in the less prestigious, less research-oriented institutions. Women are under-represented in the regular (tenure-track) faculty at research universities, particularly in the elite Ivy League Schools. Gerty Cori, who shared the Nobel Prize with her husband, was a research associate, the holder of a non-tenurable position, for 16 years (at about 10% of her husband's salary), while her husband rapidly advanced to full professor at Washington University in St. Louis. Gross discrimination does not belong in the far distant past; only a decade ago, in 1969, there were no women among the 400 tenured faculty at Harvard.

Now, at the start of the 1980's, when affirmative action has some influence, colleges and universities have few tenurable positions open to anyone. As many have noted, the recent women's liberation movement came about ten years too late for most women to reap benefits from the rapidly expanding economy of the 1960's.

Vera Kistiakowsky, in her article in Physics Today, organizes the many possible reasons for the under-representation of women in physics into several major categories, and I will apply these to women in the sciences and math in general. In addition, then, to overt discrimination, Dr. Kistiakowsky cites: 1) the "Matthew effect", 2) family and career conflict, and 3) the environment.

The phrase "Matthew effect" was coined by the sociologist Robert Merton, from the Bible quotation of Matthew: "For unto everyone that hath shall be given and he shall have abundance; but from him that hath not shall be taken away even what he hath" (Matt 13:12). The "Matthew effect" simply points out that a greater share of recognition and reward for scientific work comes to scientists with established reputations and leadership positions at research universities as compared to lesser known scientists. Thus, success rapidly breeds success in science as in other human endeavors. The characteristics making this more striking in science are the expense of scientific research today and the importance of the informal communication and professional network. It becomes easier, then, for well-known scientists to get grants and graduate students; these, in turn, increase the quality, number, and scope of publications coming out of that scientist's lab, thus attracting more grants, the brightest students and perhaps awards. Women are most often outside the inner circle, in secondary positions (in which they cannot obtain their own grants), or at less prestigious institutions, which have less money for research. Since women are at institutions and in positions that require more teaching, they have less time and motivation to do research, and less help from graduate students, post-doctorate fellows, and technicians, who do most of the actual work in science.

The importance of access to the inner circle is linked to another significant factor influencing success in science. That is the supportive function of the mentor, an older, established scientist, whose recommendation can issue the newcomer into the inner circle and who stands behind the younger scientist along the career path: graduate school, post-doctoral research, assistant professorship up through tenure. Well-meaning letters of recommendation written for women often include comments about their demeanor and psychological state, while omitting any statement about their potential as young scientists. Low expectations of a woman in science are reflected in this striking absence of any judgment about her scientific development and future contributions in her field.

A crucial function of the mentor and the "old boy" network is to label the new scientist (brilliant, promising, technically good but dull) without having to spend time actually assessing performance. People often assume that it is easy to judge a scientist's work or potential because of the presumed objectivity of science. Both the objectivity and the ease of judgment are overrated. Counting the number of publication of a scientist does not tell you anything about the quality or the significance of the work itself. Therefore, the scientific community, as any other community, bases the reputation of the newcomer upon the reputation of the mentor, and the "old boy" network determines who is the "best" candidate for a job.

Women scientists are breaking into the inner circles (in some fields) and learning how to use the system to their own advantage. More importantly, they are mobilizing their own networks and professional groups for support and increased visibility, groups such as American Women in Science.

Science is characterized in our culture at this time as a "masculine" endeavor, and it certainly is a male-dominated one. For both men and women scientists, most of the role models have been men. Women who have gone into science, particularly since the 1950's, have often been unaware of this; their role models have been seen at first simply as scientists. The degree to which women have ignored or not noticed that they are treated differently from their male colleagues is, I think, a testimony to their primary identification as "scientists" - no gender attached. For many, however, the absence of female role models eventually becomes a source of discouragement and of internal conflict. In the past, the impact of Marie Curie's fame on many girls was significant: "If she can do it, I can do it." Many highly motivated women went on for years without noticing what a rarity, a curiosity, Marie Curie was, or that her success has often been credited to her husband. This has been a common reaction to wife-husband scientific teams.

It is here, in the very powerful influence of role models on a woman's entry and development in science, that one of the major strengths of women's colleges becomes apparent. Women's colleges are still unusual in having faculties in which women are present in proportions resembling the general population. Women's colleges have much better records for young women continuing successfully in the scientific profession, particularly in the physical sciences and math. The contribution of female role models who take themselves and their students seriously, contrasts with the effects of the relative absence of women on science faculties of research universities such as Harvard.

The dearth of female role models reaches an extreme in traditional studies of the history of science. Talented and exceptional women have done science in the past in different cultures and epochs. The history is there, waiting to be written, as some historians, mostly women, are finding to their delight.

Margaret Rossiter is now completing one volume of an extensive archival study of American Women in Science 1830-1940, which should be an excellent stimulus toward establishing a heritage for women in science today.

The major problem of integrating a career with family responsibilities is one which half of all females raising children in this country confront everyday.

This conflict is created for the most part by the culture's expectations that women take the full responsibility for raising children, either directly or indirectly, and is compounded by the lack of good and inexpensive child-care facilities. In addition, the psychological conflicts arising from societal attitudes about motherhood and marriage can be considerable, although some women say that the problem loomed larger in their imaginations than in reality. Historically, women around the turn of the century who were committed to their work in science often did not marry. Those who did, increasingly through this century, often married scientists with whom they worked. Many did not have any children. Now, studies show that women scientists with one or two children may publish as much or more than their male colleagues.

Today, the special problems of a scientific career are created mainly by the rigid time sequence of the career path, which requires the greatest time and energy commitment during the years considered safest for child-bearing - that is, up to age 35. Women's problems are also compounded by the erroneous belief that science cannot be done on a part-time basis.

Thus, the traditional scientific career (as most academic careers) is inherently discriminatory against women. It also discriminates against the increasing number of men who want to share in raising their children. It has not escaped notice that many successful male scientists, in their dedication to their work and careers, cut themselves off from their families' lives.

The spectre of the family-career conflict imposes both external and internal limits on women, if they themselves accept the either/or dilemma. There is one particular "catch-22" that still fosters covert discrimination: the assumption that it is a waste of time, energy and money to train women as scientists because they just go, get married, have children and never use their training. This is simply not true today.

Studies of recent (1960's and '70's) bachelor's and master's degree holders in science and engineering indicate that, of these, 85% of the women and 88% of the men were part of the labor force in 1974. Thus, women recently trained in science were, in 1974, working or seeking work in the same proportions as the men. That is a very significant piece of information. (Unfortunately, the studies show that, compared to men, women's unemployment rates are higher, their salaries are lower, and they are less likely to be employed in their field or in science.)

What these studies indicate, as reported by Betty Vetter in Science magazine, is that women trained in science and engineering are strongly dedicated to continue working, and participation increases to 95% among women with doctorates. Women with young children, if they leave the work force at all, are returning to work much sooner than in the past.

Women are clearly demonstrating their commitment in science. A commitment is now required from our society to expand opportunities for equal participation, not just in numbers but in fields of choice, salary and status. Making short career interruptions and reentry a matter of course, increasing the flexibility of the tenure track, supporting part-time graduate and post-doctoral work, and increasing the opportunities, status and pay for part-time work in science in all sectors - academe, government and industry - would alleviate many family-versus-career problems for both women and men.

The term "environment" encompasses a wide range of factors that influence an individual's potential or actual scientific career. Cultural messages about appropriate roles for women are confusing, contradictory and still strongly rooted in the confining stereotypes of woman as wife and mother. The fact that women are working in greater numbers than ever to support themselves and their families tends simply to add one more responsibility without reorganizing and sharing others. The emergence of the "superwoman" has shifted unrealistically low expectations of women to unreasonably high ones.

These societal messages, confusing and self-defeating as they are, are heard by women and men alike, and generate what are termed the internal (in the women themselves) and external (in society in general) psychological barriers which contribute substantially to the low participation of women in the sciences. It should be no surprise that denigrating attitudes towards women - low esteem, expectations, motivation and confidence - are internalized by women themselves. According to Vetter's article:

"Commenting on the passive and dependent role assigned to women by societal values, one woman pointed out that 'This self-limiting expectation is a negative factor that must be overcome by constant internal struggle.'" (p. 34)

What are the remedies for low self-esteem and expectations, solutions to both the fear of failure and the fear of success detected in women? The 1970's have witnessed substantial change in women's attitudes and behavior. The term "consciousness-raising" may be a cliché by now, but underlying that term is an experience which prods awareness and often awakens a new view of life and its potential for the individual.

However, there is considerable resistance among women scientists (especially the successful ones) to identifying themselves as women and to admitting that they have experienced any discrimination. This is often a source of disappointment to younger women who turn to their few female role models for support and insight on coping mechanisms.

The last problem I will mention is the attitude that women are less successful in science than men because they are innately less suited to the rational, objective, creative work of good science. Few people state the notion outright, but it is implicit in the opinion that the negative influences on women could not account for their low participation in science. I propose that, first of all, the assertion that science is rational, objective and "masculine", as opposed to intuitive, subjective and "feminine", is simply a fiction that does not stand up to close examination.

Second, all the research on sex differences, if analyzed closely, tells us nothing about possible innate differences in scientific ability (whatever that is), just as it has told us nothing about differences in intelligence between males and females (although there is a long tradition of male scientists claiming objective, "scientific" proof of the inferior intelligence of women). However, the following statement from the Pulitzer Prize winning book On Human Nature by the eminent sociobiologist E.O. Wilson here at Harvard reinforces and legitimates society's prejudices: "Even with identical education for men and women and equal access to all professions, men are likely to maintain disproportionate representation in political life, business, and science." (p. 138) That is his opinion and there is no evidence to back it up, but he presents it as part of a powerful theory of human behavior.

This quick delineation of some of the negative influences on women's freedom of choice, in the context of the scientific professions, may sound very discouraging. It is crucial for us not to lose determination and vision. Obviously, many women have avoided, ignored, or overcome some of these problems. The situation in science is a little worse than in some fields, but a little better than in others.

I have heard it said that any woman who is a scientist is also a heroine. This is especially true because she did not set out to be a heroine; she only wanted to be a scientist.

I will close with a statement made by Dr. Mary Bunting, scientist, President Emerita of Radcliffe College and founder of the Institute for Independent Study, which is responsible for this symposium. She said:

I decided many years ago that I was far more interested in being a fact than in living anyone else's theory (p. 22).

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Objectivity in Clinical Trials

Sonja M. McKinlay

1. INTRODUCTION

The issue of objectivity in clinical trials is multifaceted, not all aspects being recognized as possessing appropriate solutions. The one aspect of clinical trials which sets them apart from other forms of experimentation (whether agricultural, social or in the laboratory) and lends a unique twist to the issue of objectivity is the fact that they are experiments on sick human beings. (Excluded from this discussion are preliminary clinical studies, which are usually performed on healthy volunteers).

Clinical trials are as ancient in history as the need to test out different medical remedies, although their present formalization, using statistical methods of design and analysis (developed primarily in agricultural research) has a very short history of less than four decades. The present discussion will be concerned with this recently emerged form of clinical trial, which can be strictly defined as an "experiment" (see, for example, Kempthorne, 1977). Minimally, this definition includes control of treatment-assignment by the experimenter. Further restrictions which can and should be added are discussed below.

The definition of objectivity is difficult in a positive or active sense (describing what it is). Rather, this paper will use as a working definition one which is negative or passive (describing what it is not), namely: the absence of any subjective element in a decision-making process.

Given that we have now defined the clinical trial and objectivity, the remaining sections of this paper will address two major questions:

- Where do the major decision points occur in the process of implementing a clinical trial?
- How (if at all) is objectivity introduced and maintained at each of these decision points?

2. MAJOR DECISION POINTS IN A CLINICAL TRIAL

Four major decision points can be identified in the course of implementing any clinical trial. It is recognized that other decision points may occur in some clinical trials, but these are generally subsidiary to one of these four major points, which are, in chronological order:

- (a) the decision that a clinical trial of one or more treatments is necessary;
- (b) the decision on how to allocate patients to treatments;
- (c) decisions concerning the observation of outcomes; and
- (d) decisions required in the analytical and inferential process.

Each of these points will be discussed briefly in turn.

(a) Necessity of a Clinical Trial

This decision point is the one which is least often recognized or discussed. Yet, lack of a clear decision at this initial point is the major reason why many needed trials are not initiated or, if initiated, fail subsequently.

To determine that a clinical trial is required is to recognize that the relative effectiveness of one or more treatments, when compared to no treatment or to each other, is in doubt and needs resolution.

(b) Patient Allocation to Treatments

Once the decision has been made to initiate a clinical trial (there exists equivocation as to treatment effectiveness), the method by which patients are allocated to treatments must be resolved.

This point is the inappropriate focus of continuing debate because the determination of assignment-methods highlights the conflict between the requirements of a well-designed experiment (including impartial assignment to treatments) and the clinical training of physicians which dictates that every patient should be given the benefit of the best available treatment. This debate should be resolved as part of (a) in the sense that recognition of the need for a clinical trial implies recognition that there is no "best" treatment and that, therefore, impartial assignment is not only ethically permissible but logically imperative.

Decision (b), then, focuses on the method by which this impartial assignment is accomplished.

(c) Observation of Outcomes

Treatment outcomes can generally be classified into two groups: those which involve tests or measuring instruments; and those which involve patient reports of symptomatology. Issues of objectivity are somewhat different for each of these two groups.

(d) Analysis and Inference

Decisions regarding methods of analysis and inference are generally considered in statistical terms which involve the use of statements of probability. The very use of statistical analysis is generally considered to imply objectivity and this decision point is therefore seldom recognized as a focus for issues of objectivity at all.

3. IMPLEMENTATION OF OBJECTIVITY IN DECISION-MAKING

Having now identified and briefly described the major decision points in a clinical trial, we will consider in this section the means which are available or have been considered for ensuring objectivity at each of these points.

(a) Objective Assessment of the Need for a Clinical Trial

This decision point has not been given much attention in the recent literature on clinical trials, with the notable exception of papers by Hill(1963), Chalmers (1975), and Cochrane (1972).

Three steps can be identified in this process which must all be considered, logically, if a trial is to be implemented. They are:

- (i) The rational appraisal of existing knowledge and research using generally acceptable criteria;
- (ii) The establishment of equivocation in this existing evidence;
- (iii) The ability of participating physicians to act rationally on the basis of (i) and (ii).

(1) Rational Appraisal of Evidence:

Existing knowledge and research must be evaluated according to some minimal, accepted criteria. Sets of criteria have been proposed recently and used to evaluate clinical research (Boissel and Meunier, 1971; Myberg, 1974). McKinlay (1978) has proposed five minimal criteria which are jointly necessary but not sufficient conditions for a well-designed clinical trial. Only results from research which meet these criteria can be considered admissible. The five criteria are:

- (1) The subjects included in the trial should represent as nearly as possible the population of potential users.
- (2) The comparison should be adequate to determine the relative effectiveness of the treatment(s).
- (3) The assignment of subjects to treatments should be objective (free from any possible source of bias).
- (4) The size of the treatment groups should be large enough to detect with a relatively high probability a minimal difference between treatments (if it exists).
- (5) The response(s) should be objectively measured or reproducible.

Conflicting evidence arises from the failure of past studies to meet one or more of these criteria. The two which are most frequently not met are: objective allocation to treatments; and size of the treatment groups. Trials may be adequate in all respects except numbers, thus providing insufficient evidence (compare, for example, four trials of propranolol for acute myocardial infarction, summarized in Table 1). Other studies may consist of self-selected cases and controls which possess sources of known and unknown biases, the results of which are likely to conflict with those of clinical trials. Compare, for example, the conflicting reports on the efficacy of anticoagulant therapy (Gifford and Feinstein, 1969; Lovell, 1969).

(ii) Establishment of Equivocation

The rational review of prior research with respect to acceptable criteria will lead to an assessment of the state of knowledge concerning a treatment or set of treatments. If no acceptable trials have been performed, then, regardless of the consistency of results from the studies reported, a well-designed trial is clearly required. If acceptable trials have produced conflicting evidence, then a new trial which is designed to resolve these conflicts is needed.

(iii) Acceptance by Physicians

Perhaps the most difficult stage of this initial decision process is the obtaining of physician acceptance. This requires an act of rationality on the part of professionals who are constantly required to act on hunches or impressions in the absence of scientific evidence and who are trained to assess and treat each case individually and idiosyncratically, rather than according to general rules. The practice of medicine is still an art which utilizes (albeit increasingly) available scientific evidence or innovations, much as an artist uses a new brush or the results of paint-mixing technology.

This is the stage at which the classical conflict arises between the need to establish the best treatment for significant groups of patients and the need to treat the individual patient with the best available treatment. This conflict is described succinctly by Lellouch and Schwarz (1971).

One interesting example of this decision process involved the review of prior research on the use of prenatal care as presently practiced in most West European countries and advocated in all the major obstetrics and gynecology textbooks. This review resulted in agreement between the author, research colleagues, and physicians that there presently exists no convincing evidence that prenatal care beneficially affects the course and outcome of pregnancy for all except a small, definable group of high risk women. The physicians involved in this preliminary process finally accepted that a trial of prenatal care was possible and ethical, with one group receiving the full complement of examinations and tests and the other receiving only one check-up at the thirty-second week of gestation, once pregnancy had been established. Such acceptance was essential if a trial of prenatal care was to be implemented.

(b) Objective Allocation of Patients

The only method of ensuring objective allocation of patients to treatments is to remove the process entirely from human control or, minimally, from the control of participating physicians. The unique aspect of clinical trials which has increased the difficulty of objective allocation is the sequential availability of patients,

necessitating a sequential assignment procedure. Several methods have been suggested and used, but only one has been successful - randomization. Those which have been considered and found unacceptable include:

(i) Alternate Assignment: This has involved the assignment of alternately admitted patients to one of two treatments or assignment on alternate days or according to whether the date is odd or even. The major flaw in this procedure is the ability of physicians to predict patient assignment and, therefore, regulate the timing of patient admission - consciously or unconsciously. This resulted in the assignment of sicker patients to anticoagulant therapy for example (Wright et al., 1948).

(ii) Data-Dependent Allocation: Although not as obvious as alternate assignment, this procedure also has the potential for prior knowledge of assignment which may influence timing of patient admission and thus affect subsequent assignments. This procedure has been proposed in various forms (Flehinger and Louis, 1971; Lellouch and Schwarz, 1971, for example), but does not appear to have been used in actual trials because of practical difficulties of implementation.

(iii) "Open" Randomization: This involves use of an open list, or use of sealed envelopes by the admitting physicians themselves. The potential for cheating, by skipping or reordering assignments, is obvious and difficult to avoid. This is a criticism of the otherwise well-designed trial of home and hospital treatment of myocardial infarction by Mather et al. (1976). An excellent summary of "cheat-proof" randomization techniques is given by Zelen (1974) and includes, minimally, the assignment of treatment only after patient admission and by someone who is not directly involved in the admitting or treatment process (usually the hospital pharmacist, statistician or other research staff).

(c) Objectivity in Observation

To ensure objectivity of observation, all judgmental decisions must be removed from the domain of the staff treating the patient. Successful techniques include:

- measurement and tests conducted by laboratory personnel who remain unaware of the patient's treatment
- multiple assessments of x-rays and other diagnostic procedures (using the majority judgment) by staff who remain unaware of the patient's treatment; and
- reporting of symptomatology by patients who are unaware of their treatment to interviewers who are similarly ignorant.

The first two situations are "single blind" techniques with the observer remaining ignorant of the treatment. The third is a "double blind" technique in which both the patient and observers (including treating staff in some instances) remain ignorant of the treatment. This can only occur when one of the treatments is a placebo (does not involve an active treatment).

The difficulty of instituting a successful placebo treatment cannot be overemphasized, both from a practical (Blumenthal et al., 1974) and ethical (Beecher, 1961) point of view. Current requirements for informed consent increase these difficulties and have precluded the use of placebos in, for example, surgical treatment.

(d) Objectivity in Analysis and Inference

Most researchers tend to accept the supposition that, provided the design and methods of observation ensure the production of objective data, an appropriate analysis will be, by definition, objective. The fallacy of this supposition is immediately demonstrated by statistical controversy over the analysis and interpretation of some recent trials. An obvious and interesting example is that following the publication of results of the University Group Diabetes Program (Cornfield, 1971; Feinstein, 1971).

It is seldom realized or completely understood that the statistical analysis and interpretation of clinical trials, as well as other experiments, includes two intrinsically subjective elements:

- the determination of the level of precision to be employed (equivalent to choosing the level of significance in hypothesis testing); and
- the determination of the minimum meaningful difference to be detected.

At present, there is no rational or objectively determined rule by which these two (relative) quantities can be determined. They involve the interplay of preferences of the researcher and the statistician, combined with prior experience and knowledge. Two competent statisticians, approaching the same set of data may use different criteria for precision and minimum difference determination and arrive at completely different interpretations.

For example, a difference which is found to be statistically significant at the 5% level using one test may be significant at the 1% level according to another test, depending on the amount of information used. Compounding this problem, the minimal amount of information used by, for example, many non-parametric tests may and not be acceptable to some statisticians or researchers when a parametric test which uses all or most of the information contained in the data can be used. On the other hand, the data may not have the reliability or sensitivity for a full parametric analysis - depending on the judgment of the researcher. Finally, where a 5% level is acceptable evidence to some researchers in some situations, it may and not be acceptable to others or in other situations - again depending on judgments as to the importance of the findings for future clinical practice or research.

These judgmental variations do not even address the question of what constitutes a sufficient difference between two or more treatments in order to make a decision as to the preferred treatment. For example, in comparing treatments for the common cold, reduction of the duration of symptoms one day on average, from 5 to 4 days may not be sufficient to recommend one treatment as clearly preferable to another. On the other hand, a treatment which cures even one patient who had an otherwise incurable disease would generally be preferred to the previously used ineffective (or merely ameliorative) treatments.

4. CONCLUSION

The analysis and discussion of issues of objectivity in clinical trials presented in this paper reveal an interesting and unexpected contradiction of commonly held perceptions of clinical trials (or any experimental research). The one bastion of objectivity - statistically based analysis and inference - is demonstrated to be the one point in the process of completing a clinical trial for which an objective procedure cannot be implemented. Even philosophical discussions of scientific inference and the role of statistical procedures either overlook or gloss over the inherently subjective nature of aspects of this process (see, for example, Popper, 1959; Nagel, 1968).

Renewed awareness and discussion of objectivity (or the lack of it) in statistically-based analytical and inferential procedures is required if the best use is to be made of methods presently available and new, improved approaches developed in the future. Only then can clinical knowledge, based on results of well designed, conducted and analyzed trials, begin to advance systematically and surely beyond the art of applying hunches and intuition.

TABLE 1

A COMPARISON OF REPORTED AND REQUIRED GROUP SIZE FOR EACH OF FOUR TRIALS
OF PROPRANOLOL, USED FOR ACUTE STAGE OF MYOCARDIAL INFARCTION

<u>TRIAL</u>	<u>REPORTED MORTALITY</u> <u>RATE (%)</u>		<u>REPORTED GROUP</u> <u>SIZE</u>		REQUIRED MINIMAL GROUP SIZE TO DETECT 10% DIFFERENCE IN RATES*
	PROPRANOLOL	CONTROL (PLACEBO)	PROPRANOLOL	CONTROL (PLACEBO)	
SNOW (1965)	17.0	34.8	47	46	478
BALCON et al. (1966)	23.7	25.0	59	60	373
CLAUSEN et al. (1966)	30.0	33.3	53	57	478
STEPHEN et al.	15.0	12.6	100	95	225

* $\alpha = 0.05$ (SIGNIFICANCE LEVEL)

POWER = 0.90 (PROBABILITY OF CORRECTLY DETECTING A DIFFERENCE OF 10%)

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The Cult of Objectivity in the Physical Sciences

Jill C. Bonner

Abstract

Professionals in the physical sciences are dedicated to the ideal of objectivity: in their investigations of the nature of the physical world, they strive to eliminate factors rooted in the human condition which might affect the objectivity of their theories or experiments. Such factors include subjective considerations, emotional involvement, biases conscious or subconscious, and illogical or irrational arguments. Such a philosophy requires rigorous intellectual discipline, usually acquired after years of training. It may require the abandonment of a cherished theory, nurtured over many years of research, overnight in the face of a "counterexample", i.e., the emergence of an incontrovertible result inconsistent with the existing theory. Objectivity is what gives science its international flavor, since, ideally, national cultural influences are subtracted out. Scientific practitioners take pride in their pursuit of objectivity. However, in this talk, I examine rather critically the degree of objectivity actually attained, in terms of both current research in the physical sciences and attitudes and the structure of the profession. Specifically, the objectivity of professional criteria as they affect women scientists will be examined.

I am presenting this talk not as a sociologist, behavioral scientist, philosopher or historian, but from an individual perspective, as a practicing solid-state physicist and someone who has encountered severe obstacles at several crucial points of career development. These setbacks have caused me to examine the nature of scientific research and, particularly, the underlying structure of the scientific profession, with considerable interest and a rather critical eye. I trust these personal observations will prove stimulating, and perhaps lead to further, more quantitative, research into the professional structure of the physical sciences.

I shall first sketch what I consider to be the popular view of science and then comment on the salient personality features of scientists as I have known them. This will lead into a discussion of what is understood by "objectivity" in the scientific profession.

Science is chilling to many people, since it thoroughly excludes what are regarded as normal characteristics of human nature, namely,

emotionalism, psychological moods, sudden impulses, inadvertence, incompetence and inefficiency. This view is, on the whole, an accurate perception. Specifically science is regarded as cold, impersonal, abstract, symbolic, and mathematical, with no room for emotion and error. Consequently, science attracts people who not only have mathematical aptitude and good physical perceptions, but also have very special personality characteristics. As I perceive them, scientists enjoy investigating their environment: they have a natural curiosity about physical phenomena and how things work. They enjoy problem posing and problem solving, in both a specific and a general sense. They like logic, order and having control over their lives and environment. They fear and dislike messy emotional involvement. This fear is so strong that a scientist exhibiting emotional characteristics becomes instantly suspect to colleagues, who fear that the accuracy of his* experimental data or the logical details of his theory may be unsound, i.e., that emotionalism may compromise his scientific objectivity. Scientists value reason and strongly distrust emotion. Since research scientists are, by and large, insulated from the day-to-day struggles of a commercially oriented society, they are less likely to vote job-related self-interest and are politically rather liberal as a class. Their attitudes to women and minorities are much less liberal. Their aversion to emotional extremes results not only from the non-emotional character of science, but also from the high level of abstract concentration sustained over long periods required for success in the physical sciences. Their social and sexual lives tend, therefore, to be conservative and conformist. Successful scientists have no energy left to spare for emotional roller coaster living. The important corollary is, that since scientists are predominantly men, conventional family lives accord with correspondingly conventional and conservative attitudes to women. The high level of concentration, hard work, dedication, drive, and isolation from the emotional frustrations of daily living make men scientists extremely dependent on the life-support system provided by their wives. Thus, older scientists at prestigious institutions tend to be overwhelmingly inegalitarian in their attitudes to women. Younger men, while they are more likely to realize intellectually and appreciate the inequities of the traditional female role, nevertheless find it overwhelmingly advantageous to their professional lives and success to maintain the status quo. The vital importance of wives as career-support systems seems to me to imply that discriminatory attitudes to women may be harder to eradicate in the sciences than in other professions.

Let us, however, return now to the philosophy of science itself and the cult of objectivity which makes science so strikingly different from other professions. As examples, we might take the world of commerce, the

* Since the physical sciences comprise, roughly, 90% males and 10% females, in this work the pronouns he/his and she/her will be used 90% and 10% of the time, respectively.

the advertising profession, the military and politics. None of these professions is noted for the candor and objectivity of its official utterances. The goal of all is the psychological manipulation of the public or of military subordinates. The world of science is very different. It is practical, logical and dispassionate. It transcends national and cultural differences. It imposes a rigid intellectual discipline. A theory retains its credibility only until it is demolished by a sufficiently compelling counterexample; and all theories are constantly under attack, i.e., examination in this light. For example, a theory may explain one thousand experimental observations correctly. However, if it fails on just one, it must either be discarded or radically modified. A theory which has to be constantly modified to accommodate new facts eventually loses its credibility. This imposes a considerable strain on an individual scientist who may spend years of devoted time and effort on his theory, with corresponding inevitable emotional involvement, only to see his efforts overturned overnight as new data come to light. Hence, the existence of the common phrase: "Many a beautiful theory slain by an ugly fact."

Objectivity: Success and Failures

After sketching out my impressions of science, the nature of the people who practice it, and perhaps the most salient feature of its philosophy-- objectivity, I come to the second part of my talk. All scientists agree on the importance of scientific objectivity. We now examine to what extent they succeed or fail in their pursuit of the ideal.

First of all, scientists, though they may become a little strange in the course of their professional development, still remain members of the human race and must constantly struggle with the concomitant non-objective features. Nevertheless, it must be said that overall there are some significant successes. I have already mentioned the international character of science, one of its more striking aspects. Further, there is a remarkable consensus of opinion in the field of science among scientists of all ability levels on who does good science, middling science, and poor science. This social perception is aided by a number of quasi-objective criteria, such as prestige of the home institution, number of publications, the prestige of the journals in which they are published, invited presentations at scientific gatherings, prizes, fellowships, and other honors and awards. A really innovative scientist may initially have some difficulty in getting her* ideas accepted, if they radically confound conventional wisdom, but the problems are over well within her lifetime and usually within a couple of years. Good work rather rapidly receives appropriate recognition. This is not to say that individual scientists are without bias, but an

* See previous footnote.

average over a group of five or six of them appears reasonably reliable. This is the conclusion of an evaluation of the peer-review system of grant proposals submitted for funding to federal agencies like the National Science Foundation.

Conflicts between a scientist's carefully developed scientific integrity and objectivity and his financial security may arise, however, when a scientist depends for his living on, say, a commercial institution. The general community appears to view with increasing suspicion scientists who speak out on behalf of commercial organizations on subjects like the safety of appliances or vehicles or the safety of new drugs. There is a fear that these scientists may have "sold out", and this fear is certainly not without foundation. Public suspicion of those scientists speaking out on behalf of nuclear-power safety seems to be even greater. University scientists are rarely in opposition. This may account, in part, for the continuing popularity of academic over industrial professional positions despite the relatively much lower salaries of the former.

If one looks carefully, however, subversions of scientific objectivity start to become apparent even within the academic wing of the scientific community. One phenomenon has recently been highlighted in the popular literature of the physical sciences* under the title of the "Matthew Principle" (after a celebrated quotation from the Gospel according to St. Matthew). It may also be stated succinctly in the form "the rich get richer and the poor get poorer." Suppose two young scientists start out in comparable fields and with comparable abilities. One is fortunate enough to be offered a position at a high prestige institution; the other (since permanent academic positions remain in short supply) has to make do at a mediocre institution. After a few years, the former scientist has, say, 20 publications and the latter, say, five. Does this mean the first scientist is really the better scientist? That would be the (naïve) conclusion of the scientific community. However, on looking closely, one observes that the first scientist has several advantages over the second. The higher prestige institution usually will have a lower teaching load; the students, in particular the graduate students, will be much better, support facilities including travel funds and grants-in-aid will be superior, faculty colleagues will be more active and more stimulating, and the prestige of the institution will enhance the reputation of the young scientist and help in bringing in invitations to participate in select workshops, give invited talks, etc., thus enhancing chances for successful grant applications. There is a powerful instability called, in economic terms, a "multiplier effect" at work here. Since the hallmark of good science is objectivity, individual scientists pride themselves in the "objectivity" of their scientific judgment. This example shows just how suspect that judgment might be. As presently exercised within the scientific community,

* Vera Kistia'kowsky in Physics Today, February 1980.

judgment or peer evaluation of a scientist's credentials rests solely on present achievements, without regard to the level of opportunity experienced by the scientist in attaining those achievements. Hence an advantaged scientist appears "better" than a disadvantaged scientist. Advantage and disadvantage are highly cumulative, as has recently been forcefully pointed out.*

Therefore, I firmly believe the objective criterion for evaluation of the career achievements of a scientist should not be present qualifications but rather present qualifications IN RELATION TO THE LEVEL OF OPPORTUNITY EXPERIENCED. Unfortunately, I am virtually alone in advocating this modified criterion. My fellow scientists are so convinced of the objectivity of their observations I find it impossible to modify their thinking. The problem I have to contend with is that the effects of non-objective misvaluation of, say, a young scientist are completely masked by the self-fulfilling prophecy aspects of academic selection. "The guy who gets the breaks does well; the guy who doesn't does not." Of course, it sometimes happens that breaks are given at an early stage to a real loser. In this case, success is unlikely and the scientist drops from sight. His embarrassed colleagues then tend to play down or ignore the whole thing. Hence, failures of the professional selection system are never very apparent, which enhances the complacency of the scientific profession. A further, related phenomenon with immense possibilities for abuse is the widespread use of "potential ability" as a reason for giving opportunities to young scientists who do not have the seniority necessary to establish actual track records. No criterion could possibly be less objective. Further, "potential" seems to be a prerogative of a favored subclass of young white men: women seem genetically defective in scientific potential, or so one would tend to conclude observing the scientific profession making judgments. Here, then, we have a prime example of failure of objectivity in the scientific profession which tends to affect women adversely as a class.

An even more serious problem for women scientists lies in the very structure of the science profession. The whole profession is tailored to the life style of the (white) male. For example, the prestige of the initial appointment strongly determines eventual career success, and therefore the location must be carefully chosen. Bargaining for both salary and work or status occurs at every stage of a scientific career, and frequently exploits competing offers from a number of institutions. I state the obvious when I say that women, far more than men, have their mobility and therefore also their bargaining capability restricted by spouse and family. Women are all too frequently big losers in the scientific race through this factor alone. Male scientists tend to be totally unsympathetic. They assert that obviously and objectively "that's the way that science is run." Women and any others who do not or cannot fit in must pay the inevitable price. I strongly contest this point of view. I assert that

* Jonathan Cole, Fair Science: Women in the Scientific Community

the professional structure of science should be such as to bring to the top of the profession the most able scientists, regardless of color, sex, race or other secondary considerations like mobility. This is not to say that mobility is irrelevant to scientific advancement. It is important to visit other institutions or other countries for relatively short periods of time, of the order of a few months. But this is not the long-term type of mobility that restricts opportunities for women. Suggestions for tackling this problem to give an even break to a wide class of young scientists are: (a) to put starting salaries on a uniform, nationwide basis (except for cost-of-living adjustments), and (b) to tie advancement within the institution more to actual achievement than to competitive outside offers. The establishment of grievance procedures at certain universities is also helping immobilized women.

A feature that strikes me most forcibly is the deep conviction of established male scientists, which they feel is based on unimpeachably objective and dispassionate grounds, that the present system is the best and only way to operate the scientific profession. This seems to them an utterly self-evident and objective fact. It doesn't seem that way at all to me.

In summary, therefore, I am concerned that the ideal of objectivity, which is an essential ingredient in the conduct and evaluation of scientific research itself, is being compromised in a very widespread fashion when it is extrapolated to situations involving social values and human judgment. A successful scientist who is satisfied by his level of objectivity in scientific research seems convinced he is equally objective in his judgments in other areas. This is why physical scientists appear to be the class of male professionals most resistant to the idea that pervasive discrimination and lack of equal opportunity can exist within their profession.

Report
Ethical Choices for Science Panel

Prepared from
Margarita Crocker's and Zenia Colette Edwards' summaries.

The main thrust of this panel centered around Everett Mendelsohn's discussion of "Nuclear Energy, Weapons and Scientists" and Miriam Schweber's report on "Ethics and DNA Research." Both Mendelsohn and Schweber focused on the dichotomy that exists between the acquisition of scientific knowledge and its subsequent application. Although it is clear that ethical choices are involved in deciding the uses of science, the scientist must also realize that the mere acquisition of knowledge involves responsibility. The scientist cannot become detached from his discovery, yet at this point in time, there is no mechanism to direct the uses to which knowledge is put. The role of the scientist, the government, and the public is as yet unclear. What guidelines is a scientist to follow? To whom is he responsible? These questions have always confronted the scientist, yet, in our age when improper uses of scientific knowledge could actually destroy life on earth, these considerations reach a new level of importance.

Mendelsohn used the case of the history of the construction of the atomic bomb to illustrate the need for ethical guidelines for science. He related the atmosphere which pervaded the period prior to and during World War II and the development of the atomic bomb. The bomb was invented out of fear of the spread of Nazism. Once the project got started, it gained momentum from the inherent interests of the scientists involved in exploring this new frontier.

Once finished, Oppenheimer, director of the study at Los Alamos, was able to pronounce that "science had known sin," while other members of the scientific community were putting their faith in an "optimism of progress" and proclaiming nuclear energy as "sunshine units." Victims of the atomic bomb fallout undoubtedly find little comfort in this line of reasoning. The reluctance and opposition to the further implementation of nuclear energy as an alternative to oil or coal speaks well of the ever-present controversy surrounding this issue.

Miriam Schweber reinforced many of Mendelsohn's points. For example, she emphasized that although the acquisition and application of knowledge should be ethically divorced from one another, the present situation, in which scientists are simultaneously responsible for acquiring, knowledge, regulating it and applying it, makes this impossible. This is a clear case of conflict of interests, yet at this point the scientists

are the only people technically qualified to deal with the uses to which their discoveries are put. Since biological research such as DNA research impinges directly on the biological world, the biologist must be even more careful in his research than the nuclear physicist. Schweber furthermore pointed out that these research activities coupled with the economic uses to which the acquired knowledge is put can go "deep into society" and change it.

Schweber began her report with a discussion of DNA research. She mentioned that the high rate of death by cancer especially in people between the ages of 20 and 65 gives a strong utilitarian motivation to DNA research. In trying to discover why a cancer cell loses control and keeps dividing one must first study why a normal cell stops dividing. The shock of the discovery that eucaryotes not only have RNA and DNA associated with them but actually have 30 times more DNA associated with them than they should leads biologists to the awareness of their own ignorance. Thus experiments were done in which eucaryote genes were placed into procaroyte systems where controls were applied. It was at this point that the public became concerned about the possible health hazards associated with DNA research. In Cambridge this question was seriously looked into and successfully resolved when it was shown that the organisms in question immediately die when put in contact with such substances as soap, bile, etc. Schweber was quick to point out that despite the responsible manner in which the safety question was approached and resolved in Cambridge, there is by no means a consensus as to the place of the public, the local and federal governments, and the scientific community in regulating research and its applications.

It is here that Schweber pointed out an important paradox. Originally it was believed that it was legitimate for the government to license drugs and applications of research but not to license the acquisition of knowledge. In the case of DNA research, however, a startling reversal of this policy has been witnessed. For the first time the government was putting regulations on the acquisition of knowledge but not on its use. Never before had absolute prohibitions been put on research. This ironic situation allows companies to function with no guidelines. Public money was the foundation of knowledge which the private sector is now exploiting for its own profit. Once huge sums of money are involved in applications of research, ethical considerations are harder to regulate. Scientists are acquiring the knowledge and are at the same time regulating on the national level and in the private business sector. Once the profit motive enters the scientific community it becomes harder and harder for the scientist to make responsible ethical judgments. Schweber emphatically stated that the scientists are not necessarily "on the side of the angels" and that they have been allowed secrecy for too long.

Like Mendelsohn, Schweber had no solution to the problem of who was to act as a regulator. Presently the public is not educated enough to act as a regulator, especially since any information it does have is supplied by exactly those scientists that need to be regulated (i.e. those in the forefront and those who run the companies). When "megabucks" are involved, the profit motive easily overshadows any more humanitarian motives. The unfortunate conclusion is that the acquisition and application of knowledge, at this point in time, seem to involve a highly personal choice. Each scientist must decide for him/herself since the only guideline is his/her own conscience. He/she must make his/her own "ethical choice for science."

To summarize, Professor Mendelsohn stated that twenty years ago he sat on a similar panel dealing with "Social Responsibilities of Science." Ethical choices are always with us, it seems.